JOURNAL

OF THE

BRITISH SOCIETY OF DOWSERS

Vol. V. No. 42 December, 1943

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Price to Non-Members, **1/3**

JOURNAL OF THE
BRITISH SOCIETY OF DOWSERS

Vol. V. No. 42 December, 1943

NOTICES

The particular attention of members is drawn to the Special Notice on page 295.

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Members are reminded that Subscriptions for the year July 1st, 1943, to June 30th, 1944, are now due.

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A list of books in the B.S.D. Library, that which was published in *B.S.D.J.* 35, with a few additions, can be obtained from the Editor on application.

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Copies of "Dowsing," by Captain W. H. Trinder, can be obtained from Colonel A. H. Bell, York House, Portugal Street, London, W.C.2, at 6s. 4d. for members and 8s. 4d. for non-members.

* * * * *

The first Journal of the Medical Society of Radiesthesia can be obtained from the Editor, Dr. Guyon Richards, 9 Fordington Road, Highgate, N.6, at the price of 2s. 6d., postage 2d. extra. A cheque or Postal Order for 2s. 8d. should accompany the order.

Members writing to the Editor for copies are reminded that he is a busy doctor and that the preparation of the Journal involves a considerable amount of work for which there is but little time. He therefore requests that members will kindly refrain from writing letters which require an answer unless they are about the Society, in which case they should kindly enclose postage.

This does not apply to letters sent for publication in that Journal or for answer therein.

* * * * *

The price of *Journals* to non-members is 1s. 3d.

The price of new *Journals* to members, in excess of the free number, and of old *Journals*, is 9d. and 6d. respectively.

Six free copies of the *Journal* will be given on request to writers of articles in it, in addition to the usual copy.

* * * * *

Messrs. Devine and Co. Ltd., St. Stephen's Road, Old Ford, London, E.3, supply pendulums of whale ivory with central suspension and cavity for sample, at 7s. 6d. each; also nickel-silver and copper angle rods, together with whalebone rods in desired dimensions of flat, square or circular section.

The Society's badges can be obtained from the Honorary Secretary. Owing to the increased cost of postage the price is now 1s. 8d., post free.

* * * * *

Communications for the Editor, and inquiries, should be sent to Colonel A. H. Bell, York House, Portugal Street, London, W.C.2.

NEW MEMBERS.

ANDERSON, W. R., 27 Montgomery Road, Edgware, Middlesex.
BOMPAS, Mrs. BUCKLAND, Flat 10, 24 Palace Court, W.2.
BUGLER, B. C., 47 The Hollins, Sowerby Bridge, Yorks.
CLEMETSON, Mrs. GORDON, East Lodge, Groombridge, Tunbridge Wells.
CORNWALL, E. M., 33 Holbrook Way, Bromley, Kent.
CUNINGHAM, Miss V., 47 Heath Street, N.W.3.
DAWES, A. C., 14 Belgrave Square, S.W.1.
FISHER, Mrs., Whitbourne, 535 Wolverhampton Road, Oldbury, Worcestershire.
FROST, Miss I. M., 3 Chapel Street, Woburn Sands, Bletchley, Bucks.
GOULD, A. A., 46 Clifton Road, Finchley, N.3.
HEWITT, F., 62 Rosehil Road, Burnley, Lancashire.
KEIGHTLEY, J. H., Moorfield Farm, Glendale, S. Rhodesia.
MAHADEVA, S., M.I.C.E., M.I.W.En., Public Works Office, Colombo, Ceylon.
MYLNE, Mrs, Lota House, Manby, Brisbane, Queensland, Australia.
OAKEY, P. H., Devonshire Club, Eastbourne, Sussex.
O'REILLY, H., Bingfield House, Crossdoney, Cavan, Eire.
PARKER, O. F., 55 Hillcroft Crescent, Ealing, W.5.
PRADHAN, S. R., College of Agriculture, Nagpur, India.
PRESTWICK, A. A., Chelmsford Road, Southgate, N.14.
QUAIN, J., 42 Industrial Road, Sowerby Bridge, Yorks.
RIDDEL, Mrs. D. M., 125 Cranmer Court, Sloan Avenue, S.W.3.
SHAW-MACKENZIE, Mrs., Newhall, Balblair, by Conon Bridge, Ross-shire.
SMITH, H. S.
WARNER, Miss B. C., 7 Lansdowne Road, Croydon, Surrey.

CORRECTIONS.

* Life Members.

*BRANCH, H. F., Claybury, St. John, Barbados, B.W.I.
BROWNE, O. H., Bower House, Soberton Lane, Droxford, Southampton.
CLARKE, J. A., c/o C. R. Garrett, High Street, Wheathampstead, St. Albans.
COLLINS, H. S., c/o Barclays Bank, 160 Piccadilly.
EDNEY, Lieut.-Colonel A. J., Pounceys, Brackendale Road, Camberley, Surrey.
GADRE, R. N., Assistant Marketing Officer, Nagpur, C.P., India.
HAMILTON, C. F., 50 Westhall Road, Warlingham, Surrey.
HAWKESLEY, L., 75 Victoria Road, Kensington, W.8.
JARVIS, H. W., 37 Regent Road, Surbiton, Surrey.
LINES, W. G., G. Lines & Sons Ltd., 2 Lode Lane, Solihull, Birmingham.
NASSIF, L. P. G., Farnham Royal Cottage, Farnham Royal, Bucks.
NUNAN, W., B.A., M.D., 99 Harley Street, W.1.
*ODDY, Miss M., 3 South Close, Highgate, N.6.
SYKES, E., 9 Markham Square, Chelsea.

SPECIAL NOTICE

From time to time various members have remarked that the annual subscription, which is much lower than that of any other scientific society, ought to be raised.

This point was considered in the draft for new rules which was made and circulated to members in 1939, but owing to the outbreak of war it was decided by the Council to postpone the submission of these new rules to a General Meeting till after the termination of hostilities.

The Council is now of opinion that, though it is undesirable to introduce an entirely new code of rules at the present time, good reason exists for raising the present rate of subscription and for introducing an entrance fee without further delay.

Ever since the Society was founded ten years ago the expenses have been extremely small because all the clerical work of the Society has been carried out without paid assistance by the Hon. Secretary and Treasurer, and by the President at his business premises. Such conditions cannot be expected to be permanent.

Owing to the very small annual surplus it has not been feasible to make adequate grants towards research and for the furtherance of the objects in which the Society is interested, such as the payment of lecturers, purchase of apparatus, books and so on.

The introduction of an entrance fee would tend to make membership of the Society more appreciated and should prevent the intermittent payment of annual subscriptions which is now of such frequent occurrence.

It is natural that the subscription of residents overseas should be less than that of other members, as the former do not have the opportunity of attending meetings or, in most cases, of drawing books from the library.

With a larger income it would be possible to give material aid to the furtherance of the Society's objects and also to improve the quality of the *Journal*; thereby the status of the Society would be enhanced and its usefulness much increased.

The Council accordingly suggests the following alterations to Rules II, III and IV, as now printed in every number of the *Journal*:—

Replace Rule II by the following:

II.—Entrance Fee and Subscription.

(*a*) The entrance fee for permanent residents in Great Britain is 10/6 and the annual subscription is 10/-.

(*b*) The entrance fee for permanent residents overseas is 10/6 and the annual subscription 5/-.

The subscriptions under (*a*) and (*b*) may be compounded for by the payment of a Life Member's subscription of six guineas or of three guineas respectively.

The Council is empowered to decide any special cases in connection with the payment of subscriptions.

In *Rule III* insert after the word "meetings" in the third sub. para :

"allocations of funds to promote the objects and interests of the Society."

and delete "etc."

Rule IV:

Omit the last line and insert the word "audited" before the word "accounts" in the second line.

The objects of the proposed additions to Rules III and II are to enable the Council to make grants for research and so on without the prolonged delay which would be occasioned by reference to a General Meeting, and also to empower them to decide any questions which may arise regarding the payment of subscriptions, such as whether a member who has failed to pay a subscription for one or more years may rejoin without the payment of an entrance fee, or what additional amount a member residing abroad who has paid a life member's subscription should pay on coming to reside in England. All such questions would have to be decided on their merits.

The alteration to Rule IV is required as it is not always feasible or desirable to have the accounts audited by one who is not a chartered accountant.

As it is not possible to hold a General Meeting which would be fully representative, members are asked to record their vote on the enclosed card, stating *without any qualification* whether they are in favour, or not in favour, of the suggested alterations and to post the card which accompanies this *Journal* as soon as possible.

All cards returned should have been received by the end of March. If the replies reveal a majority in favour of the alterations, it is proposed to introduce them from 1st July, 1944, so that after that date all *new* members would pay the entrance fee and all "home" members would pay the increased rate of annual subscription.

The replies only of life members and of members who have paid their subscriptions for the current year will be taken into consideration.

ANNUAL GENERAL MEETING, 1943

The ninth Annual General Meeting was held at the rooms of the Royal Asiatic Society, 74 Grosvenor Street, at 8.15 p.m. on Wednesday, October 13th.

Members present were Colonel A. H. Bell (Chairman), Mrs. S. S. Allan, Mr. W. R. Anderson, Mr. S. S. Barber, Mrs. Barraclough, Mrs. Wyndham Bates, Mrs. Bell, Miss C. H. Bell, Mr. R. Ferrer Burlton, Miss M. Cameron, Mr. H. S. Collins, Mr. A. C. Dawes, Mr. L. E. Eeman, Mr. J. Foster Forbes, Mr. G. C. N. Goldsmith, Mrs. Glanville, Mrs. Tranchell Hayes, Mrs. Jourdain, Surgeon-Captain Knapp, Miss W. B. Knight, Miss H. Leather, Mr. J. Cecil Maby, Miss C. Ouless, Mrs. F. Parker, Major C. A. Pogson, Mrs. D. M. Riddell, Mr. E. F. Ross, Miss N. Stannard, Mrs. Kingsley Tarpey, Mr. G. Underwood, Mrs. Yates.

2. The Minutes of the 1942 meeting were taken as read, a full report of the proceedings having been published in the *Journal* of December, 1942.

3. Colonel Bell read the following Report :—

Ladies and Gentlemen :

On May 4th last we completed the tenth year of our existence, and I hope you will agree that we have to some extent fulfilled the objects which we then declared to be ours. In addition to the fruitful research which has been carried out, mainly by the late Major Creyke, Mr. Maby, Captain Trinder and Mr. Franklin, by which knowledge of the whole subject comprised in the word "dowsing" has been much increased, the Society has been of use in bringing members into contact with one another and in assisting private people and institutions to obtain the services of water diviners.

Meanwhile, our membership continues to maintain its level. The number of our members shows, in fact, a slight increase, for, whereas the list published in the September *Journal* of 1942 includes 77 life members and 404 annual subscribers, the corresponding list published in the last *Journal* contains 80 life and 427 annual members, making a total increase of 26.

It is interesting to note that the first list of members, that published with the *Journal* of September, 1933, contained 108 names in all, and of those names forty stand in the latest list.

The Scottish Branch has fallen in membership from its pre-war numbers of about fifty to something under twenty, but no doubt a return to normal conditions will bring an inflow of new members.

Although the general interest in dowsing in this country is considerable, official support is sadly lacking. The reports published in several papers about the formation of squads of water diviners in the Royal Engineers appear to have been without foundation. In January last a member of Parliament

put a question to the Secretary of State for War regarding the employment of dowsers by the War Office, to which the reply was given by the Secretary of State that the War Office did not rely on dowsers in the United Kingdom and that so far as he knew the only place where they had been tried was in the Middle East, where a report of their performance showed a very small percentage of success and orders had been given that scientific geological methods only were to be used.

The information with which the Secretary of State had been supplied was certainly incorrect or incomplete, as dowsing was used in France in the early part of the war with considerable success, as I mentioned in my Report last year, and according to my information has also been used successfully in Syria. I therefore wrote to the Secretary of State, giving him some information regarding the extent to which dowsing had been used in the British Army in the past and during the present war, and pointing out that the attitude which the War Office was now taking up was reactionary. I later wrote to Sir Geoffrey Shakespeare, who is himself a dowser, who kindly put down a non-oral Question asking what trials of dowsing had been made in the Middle East and the names of the dowsers employed, and whether attention had been called to the successful finding of wells by this method in the Aleppo district. The reply was that inquiry would be made, but since then I have heard no more about it.

The attitude of the Government of India is in pleasing contrast, for I have heard of several members who have been engaged as water diviners for the Army; in fact, only a few weeks ago Colonel Edwards heard from one of our members in India, Mr. St. John Kelly, that he had been engaged to assist the R.E. in locating well sites. Earlier this year I received a letter from Mr. W. Burns, an Agricultural Commissioner with the Government of India, asking for advice as to how to start a Society of Dowsers; so it is to be hoped that it will not be long before a Society is flourishing in that country, where water is so much more precious that it is here.

Although the official outlook towards dowsing in Great Britain savours of the Middle Ages, the private outlook towards it is far otherwise. As in former years, I have received a number of applications from various parts of England for the names of water diviners; for in spite of offers of free advice to farmers and others, as recorded in a short article in the *Sunday Times* a few weeks ago, many people wisely prefer to employ the infinitely delicate radio-prospecting machine represented by the human body, which can provide information regarding the position, depth and yield of underground water with an accuracy which could never be attained from a knowledge of geological conditions only.

The archaic attitude of a certain type of scientist towards dowsing and his abysmal ignorance of the whole subject is well illustrated by remarks made at a meeting of the Brains Trust which some of you may have heard, and by an article in *Nature* of January 30th last, in which the result of what must have been a very badly conducted experiment at Guildford thirty years ago, in 1913, was referred to, in order to show that the reality of water divining had not been proved, thereby discounting the experience of several centuries, not to mention the evidence provided by numerous successful tests which had been carried out since 1913.

There is perhaps some excuse for the old-fashioned scientist who will believe in no phenomenon which cannot be put to tests of weighing and measuring inside a laboratory, and does not recognise the intangible influences which surround us and by which our daily life is ever being affected. For it is an undeniable fact that divination in the true sense of the word can be, and is, used for the same purposes to which the direct response of our nerves and muscles to radiation can be more reliably applied, and I think it should be the endeavour of every serious dowser to eliminate as far as possible this intrusive psychic element.

This is not to say that the divinatory aspect of dowsing as revealed in the location of people and objects on maps, diagnosis from photographs, obtaining answers to questions on every conceivable subject, cannot sometimes even now be put to good use; but the practice seems to be too uncertain at present to be relied on and can never be infallible until the wayward gropings of the subconscious mind can be completely controlled—a condition which can perhaps never be realised in people leading normal lives.

A part of the technique of some dowsers, which is at present inexplicable on physical grounds, is the use of colours, in one form or another, for selective purposes. In order to elucidate this phenomenon in some measure I put a special notice in the March *Journal* asking for replies to certain questions. The answers I have received so far have not been very helpful, as the most important questions, Nos. 4 and 5, have usually remained only partly answered. The point of these questions was to find out whether the dowser's reactions depend on the colour he is using or the colour he *thinks* he is using. It is probable that the latter is the truth, in which case we have an interesting psychological problem, namely, why does a dowser consistently associate a certain material with a certain mental conception of colour. Any talk of wave-lengths in this connection is, I think, out of place. To begin with, the visible colours usually met with in nature are not monochromatic, *i.e.*, of one wave-length, but a mixture of wave-lengths, which, being due in most cases to light from the sun reflected by an object, would not be the same

on a dull day as on a bright day, and in complete darkness would not exist at all. I would like to draw the attention of members to an excellent short article on Colour by Mr. Bedford Franklin which appeared in the *Journal* for March, 1942.

The Medical Society of Radiesthesia is still flourishing and issued its first *Journal* a few months ago. One of its members, Dr. Eric Perkins, gave a lecture to our Society on April 14th on the methods of Dr. Abrams. Dr. Perkins had studied under Dr. Abrams himself and has been practising his methods for many years: his lecture was, therefore, of particular interest, as it was based on personal knowledge of that most remarkable man, who inaugurated a system of diagnosis and treatment which, being entirely revolutionary, met at first with much opposition, and foolish and ignorant criticism. The lecture is being printed and will shortly be issued to our members.

During the past year Mr. Maby has continued his all-important work of research, assisted by Captain Trinder and with Mr. Franklin's co-operation on the theoretical side, with unceasing ardour and diligence.

He has carried out water and mineral survey work on a larger scale than in previous years and has conducted much correspondence with other practising members of the Society.

He has paid particular attention to methods for estimating depth and yield of water supplies, and we hope that it will soon be possible to lay down in a simple dowser's manual a more or less precise method of procedure. It seems to be well established that, in addition to the recurring reaction bands, there is thrown out by every subterranean stream or intrusive deposit a zone of depth which, by making suitable allowances for refraction due to difference of strata and by the use of a "point" as employed by the late Major Creyke, or by artificial oscillation, can be interpreted as an accurate indication of depth. This method is no doubt that which in a crude form is known as the Bishop's Rule. Further, it seems indisputable that all subterranean water in motion throws out in addition two bands on each side of the line of flow, and that the distance between these two bands, which we are calling "F" or "flow bands," is an indication of the amount of water flowing—in other words, of what the hydraulic engineer calls "cusecs" or cubic feet per second.

Mr. Franklin has been working at a theoretical explanation of these phenomena, but further experimental work is still required to elucidate the exact nature and mechanism of the electro-magnetic fields involved. However, on the practical side a fairly accurate technique has now been elaborated which Mr. Maby has himself used with considerable success and has imparted to several members of the fighting forces, notably to a Captain in the Canadian Army who is anxious to introduce dowsing as a recognized function of the Canadian Engineers.

Mr. Maby has also made further progress in respect of the analysis of various types of water possessing possibly medical value, including water from several holy wells, with the assistance of Mr. J. L. Murfin, of Leamington. He has, too, written an Introduction and Appendix for a new book by Mr. L. E. Eeman on what may perhaps be termed "radio-therapy," which when published should be of special value to all radiesthetists and to those interested in so-called magnetic healing.

In my Report last year I spoke about the Laboratory Research Fund which Mr. Maby has instituted to defray as far as possible the cost of the Research carried out for the Society's benefit in his Laboratory at Bourton-on-Hill. I mentioned that the running costs amount to between £250 and £300 annually, which sum includes such items as rent, fuel, light and electricity, insurance, stationery, telephone, instruments, travelling expenses and so on, and that Mr. Maby had obtained a loan from his Bank of £2,000, the interest on which has to be added to the running expenses. At the same time I appealed to all members to lend their support to this work of research by contributing to the Fund in as generous a measure as possible.

He is grateful to all those members who kindly sent contributions, but the result is in the main somewhat disappointing, as £52 only was received during the past year, which is less than half that contributed during the previous two years. Nevertheless, the financial situation generally shows an improvement, as the fees earned by Mr. Maby during the year amounted to £277, and these he has paid into the Fund, with the result that his indebtedness to the Bank has been reduced to £1,700.

Now, I can but repeat my appeal of last year and, in spite of the financial constriction which most of us are experiencing, ask our members to be as generous as they possibly can be, in supporting the sacred cause of scientific progress.

The *Journal* has been produced on the same lines as hitherto, but the length has been reduced to about 36 pages.

As Editor, I am grateful to all those who have assisted in maintaining the *Journal* by sending contributions, namely: Mr. Belfield, Mr. Benham, Mr. Franklin, Mr. Austin Browne, Mr. D. O. King, Mr. Langelaan, Mr. Maby, F/Lt. Mackay, Squadron Leader the Rev. W. E. L. May, Miss Penrose, Rev. M. C. Potter, Mr. F. Danvers Power, Mr. Vaudeau and Mr. Wethered.

It is interesting to note that five of these contributors were resident as far afield as Rhodesia, the West Indies, Tasmania Australia and the Argentine.

The Library, from which books are sent on loan to members in Great Britain and Northern Ireland, has been fairly well patronised, 63 volumes having been issued during the past year.

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BRITISH SOCIETY OF DOWSERS

Financial Statement for Year ended 30th June, 1943

RECEIPTS.

1941-42. £ s. d.		£ s. d.	£ s. d.
	Brought in—		
	Cash at Bank ..	20 12 1	
	Defence Bonds ..	235 0 0	
279 1 0			255 12 1
101 0 6	Annual Subscriptions		112 9 8
8 19 0	Life Subscriptions		1 13 0
7 1 7	Sales of *Journal*		8 7 7
1 3 6	Sales of Badges		1 2 7
—	Special Fees		10 10 0
7 10 0	Interest on Defence Bonds ..		6 13 11
5 6	Interest on P.O. Savings Bank ..		6 —
£405 1 1			£396 8 10

PAYMENTS.

1941-42. £ s. d.		£ s. d.	£ s. d.
22 0 8	Postages and Cheque Books ..		22 7 0
63 18 3	Printing of *Journal*		87 11 0
3 10 7	Printing and Stationery		16 2 9
4 10 0	Meetings		4 7 0
50 0 0	Special Grants		—
5 5 0	Audit Fee		—
4 6	Loss on realization of Defence Bonds		—
	Balance, 30th June, 1943—		
	Cash at Bank ..	46 1 1	
	Defence Bonds ..	220 0 0	
255 12 1			266 1 1
£405 1 1			£396 8 10

H. M. EDWARDS, *Hon. Treasurer.*

I have examined the above Receipts and Payments Account with the Books and Vouchers and certify it to be in accordance therewith.

July 26th, 1943.

A. CECIL STOUGHTON.

Now we come to the Accounts.

A financial statement was published in the last *Journal* and copies of it have just been distributed.

You will see that, starting with a balance of £255 12s. 1d., our closing balance is £266 1s. 1d.

The actual receipts during the year were £140 16s. 9d., as against £126 0s. 1d. in the previous year, whilst the actual expenditure during the year was £130 7s. 9d., against £149 9s.

The item of ten guineas amongst Receipts, under the heading "Special Fees," was for a broadcast on dowsing to Portugal which I referred to in my last year's Report.

The item of £87 11s. amongst Payments includes the cost of the production of five *Journals*, whereas the corresponding item in the previous year included only three *Journals*. The item for Stationery is larger than before owing to a purchase of envelopes.

Through the kindness of Mr. Cecil Stoughton, the Accounts have been audited without a fee.

I do not think that any other remarks are called for.

4. The adoption of the Accounts, proposed by Mr. Ross and seconded by Captain Knapp, was carried.

5. A vote of thanks to Mr. Cecil Stoughton, for kindly auditing the Accounts, proposed by Mr. Eeman and seconded by Mrs. Barraclough, was carried.

* * * * *

The meeting was followed by tea, after which Mr. Eeman delivered an address, which is printed below, on "Co-operative Healing and Reactions of the Human Body to the Frequencies of Drugs and other Substances placed in Series in the Relaxation Circuit."

CO-OPERATIVE HEALING

By L. E. EEMAN

Mr. Chairman, Ladies and Gentlemen,

It is one of our most hallowed conventions that anyone who calls himself a lecturer enjoys the privilege of speaking at length to numbers of learned people, whilst they struggle to keep their ears open and their mouths shut.

However, to turn the privilege of lecturing into a true pleasure, the lecturer must feel that his audience share with him at least a background of common interest, knowledge and belief. And here I am particularly fortunate, for you already hold wide open for me a number of doors, many of which would be barred and bolted in my face in circles where even dowsing itself is looked upon as superstitious.

You are familiar with the notion that each form of matter is capable of radiating its own specific or atomic frequency, whether it is in itself radio-active or only broadcasts its "signature tune" when it is suitably excited by external energies.

You know that drugs, though variously altered by the process of digestion, have a specific effect on the body; that although the bloodstream irrigates impartially the whole of that body, drugs act only on specific parts of it, and act on these only in specific ways. You also know that although this specific action of drugs has been axiomatic for generations, it has never been satisfactorily explained.

When under medical treatment, you have demonstrated this specific action in your own bodies. In addition, as dowsers, you have also demonstrated that the neuro-muscular system reacts automatically and in a specific manner to the radiations of specific substances, and that it does so whether these substances are situated under or above ground and outside or inside of living bodies.

Using the electro-magnetic analogy, most of you accept the bi-polarity of the human body, vertical, lateral, and dorsi-frontal, and some of you may remember a demonstration which I gave in this room earlier in the War. In this, I showed experimentally that there is polar opposition between the Head and the base of the Spine, and between the Right hand and the Left; and that, if one adopts the convention that the head and the right hand of right-handers are positive to the base of their spine and left hand, the reverse is the case with left-handers.

I also made it clear that when one links polar opposites by means of electric conductors, and thus establishes what I term the "relaxation circuit," one obtains progressive and automatic relaxation of the voluntary muscles of those in circuit, stimulation of their functions, and eventually sleep of high recuperative value, and that the reverse connections produce progressive tension of the voluntary muscles, inhibitions of function, and wakefulness.

I have here four of the deck chairs which I use for the treatment of one or more patients. At the back of each chair there are two mats of copper gauze linked by a copper wire, one mat for the head and one for the base of the spine. The head-mat is connected by copper wire with the patient's left hand, and the spine-mat with his right. When working with groups, all heads are linked together, and so are all spines, an arrangement which is similar to that of wireless valves in parallel.

When there are both right- and left-handers in a group, the right-handers are placed at one end of the line and the left-handers at the other, and the wires linking the heads and spines are crossed at the point of junction of the two groups, which preserves the correct linkage of polar opposites.

After I had placed myself in circuit almost daily for a number of years, either with one subject at a time or with groups of them, I observed not only the basic fact that linking polar opposites promotes automatic and progressive relaxation, and polar similars automatic and progressive tension, but also :

(A) that a subject in circuit can, by his own feeling of increased well-being, detect the moment when any other subject in circuit with him falls asleep.

(B) that he can by his own feeling of increased well-being detect the moment when a woman in circuit with him who is going through the menopause enters one of her "flush" or "heat" periods, and by a feeling of chill in himself when the "flush" or "heat" ends.

(C) that he can by his own feeling of increased well-being and warmth detect when the fever of a patient in circuit with him is high, and by his feeling of chill when it has fallen.

(D) that the linking of a healthy subject with a fever patient lowers the temperature of the fever patient, but only does so down to a given point, say from a "high" of 104° to a "low" of 101°, when a "no reaction" state is reached, just as though potential had disappeared.

(E) that when to this pair a third subject is added who is convalescing from the same fever as the patient's, and is therefore immunised against that fever, the patient's temperature falls below this previous "low."

(F) that a trained subject can, by his own peculiar and appropriate feelings, identify fairly accurately the condition of his fellows in circuit, striking examples being fevers, alcoholism or drug addiction.

(G) that the greater the number of subjects in circuit, the less is the disturbance caused to his fellows by one patient or addict, and the quicker the sharing of a burden produces relief and well-being in all.

(H) that there are clear antagonisms between different morbid conditions or dispositions, such as, for instance, a rheumatic

tendency and a susceptibility to certain infections, and that, therefore, the sufferers from antagonistic affections act mutually as healing sera or vaccines, &c., and that it appears wasteful, if not actually harmful, to assemble in one circuit victims of any one type of complaint exclusively.

(I) that the echoing by one organism of the physical activities of another in circuit with it, and the more effective healing of a physical complaint by a group of subjects than by a single one, are both paralleled by a similar echoing and mutual healing of psychological conditions, although the mechanism employed, *i.e.*, the relaxation circuit, appears to be purely physical.

(J) that remarkably clear results are obtained in the circuit in experimental telepathy, even with subjects entirely unversed in the subject, and this again, despite the fact that the relaxation circuit appears purely physical, though it clearly suggests the wireless aerials which are so helpful to the good functioning of our radio sets.

I may mention here,

First: that in making all the experiments on which the above and many further observations are based, I invariably took all the classical precautions to ensure their scientific validity.

Secondly: that, in particular, I always guarded myself and my subjects against suggestion, auto- or hetero-, direct or telepathic, and

Thirdly: that the majority of my experiments have either been repeated in the presence and with the participation of doctors of medicine, physicists and other scientists, and that quite a few have been repeated independently by some of these gentlemen, with the usual precautions, with the help of varied measuring apparatus and with positive results.

Nevertheless, all my efforts of the last 24 years to interest official medicine in my researches and to induce them to repeat be it only one of my experiments have either been ignored or dismissed without investigation.

I will now give you three illustrations of the points I have so far enumerated, and I will take these from the MS of a book entitled: "Co-operative healing," which I hope to publish soon after the War:

(1) I placed myself in the relaxation circuit on five consecutive days with a case of toxaemia. The first circuit produced a marked improvement in the patient, the second gave less, the third less still, and the fourth and fifth produced nothing beyond a sense of relaxation and rest in either the patient or myself. Was the patient cured? NO. She had rapidly improved up to a point, but there she had stopped. At the end of the fifth day, and unknown to the patient, I got myself inoculated against her condition. I suffered mild discomfort and febrile disturbances for three days, and in the evening of the third I again placed

myself in circuit with the patient. Well within the first minute she exclaimed with rather frightened astonishment: "What are you doing to me? You are burning me!" For over half an hour the patient remained very flushed and the "burning" feeling continued. Throughout I felt a rather pleasant glow similar to that I had experienced when in circuit with fever and menopause cases, and certainly suffered no harm. The patient reported a greater improvement than she had experienced from any previous circuit! After a further three periods in the circuit nothing more was happening! Was the patient cured? NO, for her progressive arthritic condition of many years standing had caused mechanical damage that nothing could have offset; but YES, in the sense that from that day it remained static for a long period.

(2) After three successful circuits with an infectious fever case, the no-reaction state is reached. Again, inoculation of the normal subject brings back significant reactions, but within three circuits the no-reaction state is reached once again. Was the patient cured? YES, for there was no return of fever. Was the cause removed, or had the patient been taught to produce suitable anti-vibrations or anti-bodies? I answer this double question with another: How is this double question answered when sera or vaccines, administered in the ordinary manner, have not only defeated an infectious fever but immunised a patient?

(3) An ether addict, the first I had ever met, almost overwhelms me with the potency of her ethereal breath! I make her lie down and she relaxes with the greatest ease, as might have been expected. With the idea of helping her, but very much in the dark as to what may happen, I place myself in the relaxation circuit with her. Almost at once I begin to feel sick, weak and mentally disturbed, but maintain the circuit out of sheer fascinated curiosity. The patient appears quite well, but her breathing is rather strong. Suddenly, she leaps off the couch, rushes to a deep armchair, collapses in it and for several minutes she takes enormous panting breaths which shake her whole trunk at the rate of at least 24 to the minute. Just before her leap I had begun to ask myself whether devotion to science demanded that I should die in order to make a drug addict breathe deeply, and when she broke the circuit, though relieved, I remained quite limp for a few minutes. And there we were, each aware of the other's peculiar behaviour, but much too preoccupied with his own salvation to care much about the other's purgatory. Before she left, the lady made her opinion of me as clear as an ether-fuddled brain could have done. Her dulled eyes had vaguely seen copper wires and copper gauze mats, and the next post brought me an angry letter from her husband. Quite obviously, "it was criminal to pass powerful

electric currents through the delicate nervous system of a sensitive and highly-strung woman, and I was very fortunate not to find myself in court for manslaughter!" No apology was offered for passing ether frequencies through the nervous system of an unsuspecting investigator!

After this case, I treated drug addicts and alcoholics with great respect, and I broke the circuit for a while as soon as my own reactions became too strong. In time, I found that the more subjects I had together in the circuit the less I had to fear and guard against, both for the "abnormal factor" subject and for the others. What had meant shattering breath for one and collapse for the other, meant only mild narcosis for the many, followed by speedy return to normal and wellbeing for all.

By the end of 1927, having repeatedly checked my facts with the greatest care, I was convinced that when an "abnormal physical factor" was present and active in one of a number of subjects in a relaxation circuit, the other subjects reacted in a manner similar to that in which they would have reacted had this factor been present and active within themselves. Every experiment I have made since then has reinforced that conviction.

The facts, if confirmed by hospital tests, might revolutionise medicine, and lead to a new conception of vital and morbid processes; but I realised that I had barely glimpsed the edge of a vast jungle of facts and that it would be better not to attempt to penetrate this wilderness alone. However, I failed in all my efforts to induce either Doctors or Physicists to join me in my researches, and so, I had to explore unaided.

I knew that abnormal factors, such as the germs of diseases, drugs and alcohol, could be found in sealed tubes or bottles, in pathological laboratories, in pharmacies, or in public houses respectively: but I had yet to hear that pathologists reacted to the bugs in their test tubes, or that pharmacists cured their own headaches by sitting near their bottles, or that publicans were ever made "happy" by the mere proximity of their flagons of cheer! These abnormal physical factors in sealed containers were not spontaneously radio-active, although each did possess its own specific (or atomic) resonant frequency. Not being radio-active, they could not, unassisted, radiate their own frequencies, and it seemed unlikely that the copper wire I used in my circuits could by itself explain the fact that these frequencies were nevertheless propagated from the body of the subject in whom the abnormal factor was active to those of his companions in circuit. Nor could this copper wire account for the further fact that the reactions of the "companion bodies" were themselves specific.

I therefore formulated the working hypothesis that either the subject in whom the abnormal factor was active or those in circuit

with him, or both, radiated the equivalent of a "carrier wave," and that this, whether it originated in these human bodies or outside of them, impinged on the abnormal factor, took on its specific frequency and then propagated this frequency through the whole circuit. There, nerves, organs, muscles, germs or cells, &c., of similar frequency—and *only* these—would appropriately react to the abnormal factor. It seemed evident that if this working hypothesis, or one similar to it, were approximately correct, it would make no difference to my results whether the abnormal factor were located in any one of the bodies in circuit or were placed in series in any one of the lengths of copper wire which connected these bodies with each other.

Unfortunately, this hypothesis of a human carrier wave was so unorthodox that none of the medical men to whom I mentioned it would give it a moment's consideration. However, I was irresistibly impelled to follow it up and was encouraged to do so by the fact that it could be tested in a few minutes and at the cost of about one penny. I borrowed two hairpins, put them in my pocket together with a five grains tablet of sodium salicylate and my lengths of copper wire, and called on a patient who was then running a high temperature. On arrival, I dissolved the tablet in a tumbler full of water and fitted my two hairpins on the rim of the tumbler.

After taking the patient's temperature, I connected my Left hand with her Head and my Right hand with her Spine and remained in circuit with her until a few minutes after we had reached the "no-reaction" state. Her temperature had fallen from 103.5° to 102°, and I knew from past experience that once the "no-reaction" state had been reached a further fall could only be obtained by the addition of either a new subject to the circuit or a new abnormal factor to one of the subjects already in circuit. I tried a third way: I cut the copper wire that linked my Left hand with my patient's Head and fastened each one of the loose wire ends to one of my hairpins and thus completed the relaxation circuit through the solution of sodium salicylate. Within a few seconds clear and specific reactions reappeared in both the patient and myself, and when we again reached the no-reaction state, her temperature was barely over 100°. Less than an hour had elapsed since I had first completed the relaxation circuit.

Although I had expected this very result, this one hour had comforted me a great deal, but I naturally wondered whether my observations had been sufficiently careful and accurate and whether phenomena had truly occurred as I believed they had. Further, if they had, was the conduction of specific frequencies their only possible explanation? Coincidence, suggestion, telepathy and even clairvoyance all had to be reckoned with. I will merely state here that before many months had elapsed I

had made many and varied "contra" experiments, both with sick and with normal subjects, with and without their knowledge and with different drugs and other substances, and that these further tests had left no room for doubt. I do not here describe them, for I will later relate in detail a series of simpler and, if possible, more conclusive experiments, which anyone interested in the problem may repeat, as often as he likes, with the certainty of obtaining significant results.

In May, 1936, Professor J. B. Rhine, of Duke University, U.S.A., the well-known author of *Extra-Sensory Perception* and other works, did me the inestimable favour of introducing me to Mr. J. Cecil Maby, a physiologist and physicist, on whose knowledge and experimental skill I need not enlarge in this company.

When Maby had called on me on the morning of the 27th May, 1936, he had promised that he would endeavour to give me instrumental demonstration and quantitative measurements of some of the facts I had put before him. He had also warned me that I might have to wait two or three years, or more, before his own programme of research would allow him to investigate scientifically my hypothesis of the "Abnormal physical factor in the relaxation circuit."

After Maby had left me that afternoon I could hardly get over my amazement at the fact that when I had outlined my hypothesis and described some of the experiments on which it rested he had not immediately pooh-poohed the whole thing. That was a distinctly novel experience, and I felt that it was worth waiting a long time to have my observations checked by one so open-minded. When, a few months later, he gave me the promised instrumental demonstrations and measurements, I reminded him of the "Abnormal factor" hypothesis, but he told me that he still had so much important research work to complete that he could not hope to tackle my problems for at least another two years. He made it easier for me to be patient by suggesting that when he would reach a certain stage in his investigations they would probably link up with my own. Then, the observation of similar or identical phenomena from two different points of view would give additional significance to our findings.

As time went on Maby and I occasionally stayed with each other for a few days, and we thus kept abreast of one another's progress. Towards the middle of 1939 he was getting near that stage of his work at which it would link up with mine, but the War intervened and gave him new and urgent duties, and thus it was that the first of the tests we had planned could not be made until I spent with him the week-end of April 26th to 28th, 1940. We had some difference of opinion concerning the first group of tests we should make in his laboratory. We agreed

that he would put me "in series" with different drugs, all of which would be unknown to me, and that he would write down his observations on my visible reactions and my descriptions of my symptoms whilst in series with any drug, and that at a later date he would reverse these respective positions. However, whilst I contended that the radiations emitted by my body would provide the "carrier wave" required to propagate the specific frequencies of the drugs in circuit with me, Maby argued that whether I was right or wrong, the introduction of artificial energy to the circuit would tend to "boost up" my reactions, and that if these were not marked enough for registration with this additional energy they would not be so without it. Although it might have been argued that the reactions promoted by the artificial energy itself might be strong enough to mask those occasioned by the drugs, I agreed to do as he suggested, on the understanding that at a later date we should do tests with the human body itself as the only provider of the "carrier wave," the very conditions under which I had so far done all my experiments.

For a description of what resulted I cannot do better than quote from a long letter which Maby wrote on the 4th May, 1940, to a physicist colleague, who had been associated with his researches for many years.

BIOPHYSICAL LABORATORY,
BOURTON-ON-THE-HILL,
GLOS.

May 4th, 1940

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Conduction of Specific effects of drugs through wires

I should like, later, when things are more certain and some of these effects have been checked on other subjects, to write up a special paper with Eeman, stating the remarkable results of our experiments together last week-end, when he got every specimen virtually right in terms of its specific physiological effects. But to do so would be premature at present, in case—however improbable this may appear—he was acting *telepathically*, in part at least. He will be very cross with me for mentioning that possibility, but the fact remains that it must be guarded against as the first criticism of the psychical research folk. Nor will doctors swallow such strange results without a tough fight. So that we must be absolutely certain before putting anything on public record. . . . . On the other hand, telepathy is seldom so sure as this.

Well, what we did was to arrange an electric chair in which the patient sat on one (copper gauze) electrode, while the other was under a thin (partly conductive) pad behind his head. These were *positive* to head and *negative* to base of spine (or hands and feet as well) in the approved manner, and connected to the two H.T. secondaries of an induction coil at several thousand volts tension. The interposed resistances were, however, so high that only a *very* mild sensation of faradisation resulted—scarcely perceptible. The drug or other substance to be conveyed to the patient was then included in one output lead, in series, as an aqueous solution. Eeman was the "patient," and he had no means of knowing what I was administering to him electrically. (See qualifying remarks below).

In these circumstances he not only picked the positive-to-head arrangement as being most soothing and beneficial, but also made remarkable detailed statements on his sensations and bodily reactions, including blood pressure and circulation, respiration, muscle tone, salivation, gastric and other gland action, neuritic pains, &c. And all these *in every instance* were suitable to the nature of the medicine—though the latter had only been administered in the form of electric oscillations and radiation. In some instances he even suggested the correct element or compound, such as "iodine" or "a narcotic" (actually these were chloroform and alcohol in the latter specimen used). But the trouble here is that, though the specimens smelt very little and were a long way off, it is conceivable that hypersensitivity was (even unconsciously) at work, guiding him. And speaking for myself and my wife, we do not yet manage to obtain the detailed sensations that he did, even when we know the answers! But as I get *some* specific distinctions along the same lines as Eeman when sufficiently receptive or in need of the tonic, and have had one or two very marked (and *unexpected*) physiological reactions to longish doses of this sort, I shall give him the benefit of the doubt pending check experiments—especially as he is experienced at this sort of thing beyond most people, and, perhaps, super-sensitive.

Still, I think it is essential to treat actual complaints of long standing by such methods, watching for real improvements before coming to any conclusions. Meanwhile, my own experiences, which I cannot list here, tend to confirm Eeman's. So I hope that all may turn out all right. One also has to remember that much of the "Electronist" or pathometric technique and the work of the short-wave therapy people (in which you are now interested) is on similar lines. So that it all really stands or falls together. And we have a great opening if things go well.

P.S.—I have got the upstairs room all fixed up now as a special laboratory for this stuff, and have made further arrangements since Eeman was here, such a magnetic gadget to show the general phase of field at any time and a permanent arrangement for electric treatment in terms of these h.f. radiations.

I will make a few comments on this letter.

(1) I was not "cross" with Maby for mentioning telepathy as a possible explanation of the facts, for it was one of the factors one had to take into account at all times. As experiments suggest, one can never exclude telepathy, for it appears to be a continuous process between human minds, though one variable both in efficiency and degrees of consciousness. But that it was not the governing agent in our tests is proved by one of the blind-tests which I describe later on, and in which telepathy was proved inoperative when put in accidental conflict with the specific frequency of a particular drug. Before it can be given credit for the results described by Maby, it must be shown that the reaction produced is in all details that which the operator "thinks" the drug ought to produce, even when he is mistaken either as to the properties of the drug or as to the actual drug in circuit. When this test is applied, telepathy is shown to play no part worth mentioning.

(2) I would underline Maby's remark "On the other hand, telepathy is seldom as sure as this." It seems that "telepathy" and "abnormal factor radiation" both need "interpretation" for their elucidation, unlike "clairvoyance" of an objective

reality which only requires "description." That my results were more "sure" than those of average telepathy, appears to me to be due to the fact that a complex mental image may involve several nerve centres, organs and muscle groups of different frequencies whose many radiations must be integrated and interpreted by the percipient, whereas the specific frequency of a single drug may only cause a single nerve centre of the same specific frequency to react. After this only, can the general physiological effects of that single reaction develop progressively in the subject in circuit, and be observed and "interpreted" by him.

(3) I underline Maby's "*in every instance.*" The tests made exceeded a dozen, and I must leave it to a senior wrangler to work out the odds against such 100 per cent. results being obtained by "chance" and in such circumstances.

(4) The possibility that hyperacuity of my sense of smell may have enabled me even unconsciously to "guess" at a drug in my vicinity cannot be excluded, but I must point out that the drug actually in circuit with me was near the induction coil, several feet away from me, whereas there were many other test tubes on a shelf just behind me, most imperfectly sealed. At least some of these must have smelt and bombarded my hyperacute proboscis with "contra" suggestions.

(5) In Maby's P.S., I note "electric treatment" as evidence on our difference of opinion concerning the necessity of providing artificial energy, and I reassert my belief that no such energy is required. My conviction on the point rested on hundreds of successful experiments, not one of which had involved artificial energy; but it was natural that Maby, who had worked for many years with electrical apparatus of many kinds, should have preferred to test my theories on his customary lines.

(6) I thank Maby for his strict scientific care in the gathering of facts. This severe habit, from which I have hardly ever known him to depart, was to me a guarantee that he had been scientifically convinced by our experiments, or he would hardly "have got the upstairs room all fixed up as a special laboratory for this stuff." Here at last was a scientist, an experimental physiologist and physicist, who had not only been willing to test my "abnorm al factor" hypothesis but who had found my facts solid and significant, who would repeat my tests with other subjects and devise "contra" experiments on any objection that might be raised; and who in all his work would be actuated by one motive only: the love of truth. With his support, I could resume my attempts to interest Doctors in my experiments, and I invited a number of them to attend a demonstration which I had planned for the afternoon of the 10th July, 1940. Few could accept a war-time invitation, but the few who did come were genuinely interested and open-minded. They were: Doctors G. Calver,

G. Kelham, H. Munro, H. Parsons, A. R. Redfern, T. S. Rippon, E. A. Wharton and H. Wright. This demonstration was also witnessed by a few distinguished non-medical men, amongst whom were L. A. G. Strong, the author and critic; P. Metman, a distinguished psychologist; and R. Miller, a lecturer on dietetics.

A second demonstration given on the 24th July, 1940, was again attended by Doctors G. Calver, G. Kelham, H. Munro, T. S. Rippon and H. Wright, and they were joined by Dr. and Mrs. L. J. Bendit, Mr. and Mrs. Metman, and Miss Joan Fry, amongst others. I had planned further demonstrations for the autumn and winter evenings of 1940, but unfortunately German Air Raids made such meetings "difficult"!

For each of those two demonstrations I had four subjects in the "relaxation circuit in parallel," and drugs were placed "in series" in the lead which connected all left hands with all heads. None of the four subjects could have had any normally acquired knowledge either of the nature of the drugs that were being tested or of the doses involved. Subjects were changed about, some of the Doctors acting alternately as subjects and observers; the drugs or substances tested were each kept in circuit for two minutes, during which the onlookers observed the breath, circulation and other reactions of the four subjects, and only after the latter had described their own symptoms did I disclose the name of the drug tested. The reactions observed were so similar to those which would have obtained had the drugs been administered internally that some of those Doctors, who were not at first prepared to accept my hypothesis of the conduction of high frequency radiations by wires, urged that I must have been suggesting the correct reactions to the four subjects by telepathy. I then asked one of the Doctors present to place in the circuit drugs chosen by himself, unknown to me, and with myself as one of the subjects. When he did so, results as appropriate as the previous ones were obtained. It was then objected that Dr. G. Kelham, who was placing the drugs in circuit, knew what these were, and might himself, unintentionally, promote the appropriate reactions telepathically. Dr. Kelham then took up samples at random, and "blind": the onlookers and the subjects observed the reactions these produced and then only did Dr. Kelham ascertain which drug he had placed in the circuit. All the reactions observed in circumstances which excluded both telepathy and suggestion were again similar to those occasioned by internal medication. This left "wire-conduction of H.F. radiations" as the only reasonable explanation of the facts.

Five points should be noted:

(1) None of the subjects had ever before either taken part in or witnessed my tests.

(2) No artificial energy was used.

(3) The reactions were all obtained inside TWO minutes, that is, much more rapidly than internal medication could have produced most of them.

(4) They were as marked as those which would have been caused by the internal administration to one patient of one adult dose of the drug as defined by British Pharmocopœia, yet my doses were all less than one tenth of one adult dose, they were administered by conduction, and for TWO minutes only, and they were shared by FOUR subjects.

(5) Had these drugs been administered for curative and not for experimental purposes, the circuit would have been broken and the treatment interrupted as soon as the physician had satisfied himself that he had obtained the desired result. Doses would have been measured in terms of time and not of weight; they would have been graded and spaced according to ascertained results; overdoses would have been unlikely and the patient's system would not have been called upon to eliminate insoluble, unmeasurable and harmful residues.

The relaxation circuit in parallel was an admirable means for the demonstration both of the conduction of H.F. radiations and of the efficacy of the specific atomic frequency of one substance radiated to many subjects in one circuit; but, it was needlessly complicated when only the demonstration of conduction itself was aimed at. For that purpose, only one subject in the relaxation circuit by himself was required, with the drug to be tested placed in series between any one of his polar opposites, such as his R and S, his H and S, his L and H, or his L and R. The latter was the circuit I had already used to my satisfaction in hundreds of experiments, and which I had wanted Maby to test when I had stayed with him, and when he had, in fact, used the H and S circuit, and added to it the artificial energy which I thought superfluous.

Reduced to its simplest form, this circuit only requires: one subject, two copper handles, two six-feet lengths of copper wire, a dozen test tubes, or small glasses or bottles, each fitted with two electrodes (hairpins will do), twelve different drugs each held in one of the twelve test tubes, and, of course, a little patience and interest. The subject relaxes on the back, holding one of the handles in each hand. A friend, who is to act as observer and note-keeper, places the twelve test tubes on a table a few feet behind the subject's head, and out of his sight. He also connects the two loose ends of the copper wires, thus completing the relaxation circuit for the subject, who, after a few minutes, feels relaxed, warm and comfortable, and breathes regularly and slowly. When the subject's behaviour has been stable for a while and has thus provided a "control," the observing friend takes one of the test tubes at random and blind and, without disclosing its contents to the subject, he places it "in series"

between the two copper wires, thereby closing again the relaxation circuit through the drug to be tested. The drug remains in series for not less than two minutes, during which the subject describes his symptoms, and these are noted by the observer together with any reactions he may have detected.

In August, 1941, a young chemist consulted me. During our first two meetings, we covered a good deal of ground experimentally, and the third time he came, I placed him in circuit with ten different drugs in succession without giving him any hint of what they were. Two minutes per drug, two copper handles, one in each hand, two bits of copper wire, two electrodes in each test tube! During the first two minutes, frankly puzzled, he observed and described many new and unexpected sensations. When time was up I asked him: "Was that what you would have expected from this?" and showed him the label on my test tube. He replied: "Exactly." Nine times I asked him that question and nine times he expressed his progressive amazement. I was more thrilled than I could describe. At his first attempt, 90 per cent. success! Would I get a hundred? No. I didn't. For the last drug, he was dead wrong in everything; his words, his quiet expression, his restful almost sleepy eyes, his peaceful breath, everything! I was very disappointed and asked him if he was quite certain that he had felt all that he had described, and he assured me he was. Resigned to that 90 per cent. when the hundred had seemed so near, I said: "Well, anyhow, that was not what you would have expected from this?" and he replied: "It was, exactly! But you see, I am one of those people who have what is known as caffeine tolerance." He then told me that "the caffeine test had been more convincing and enlightening to him than the other nine put together, that before it, he had been inclined to credit me with abnormal powers of telepathic suggestion, but that when his reactions had proved diametrically opposed to my expectations, he had realised instantly that H.F. radiations and not telepathic suggestion offered the only reasonable explanation of the facts. And this, in the absence of all artificial energy! That meant that he would have to think anew all he had ever thought in relation to drugs and their actions! Would I mind if he took some time digesting these remarkable new FACTS?"

Before my patient left me, on the 23rd August, I asked him if he would do me the favour of sending me, say, half-a-dozen different drugs labelled A, B, C, and so on, so that I could have no idea of their nature. He kindly promised to do so. A few days later, there arrived a large parcel containing twenty-eight different bottles.

The 28 bottles were labelled "A" to "Z" and "Alpha" and "Beta," a code of which our chemist shared the key only with a friend of his. Illustrating his keen experimental sense,

he had included amongst them some drugs of which he himself did not know the effects. He had done so without telling me which were which, and had decided that he would only look up these drugs in his reference books AFTER I had sent him my reports on them. He had thus answered in advance the objection of telepathy.

This was indeed collaboration such as only Dr. Helena Wright, J. C. Maby, and my assistant, Miss Cameron, had given so far.

In his letter, he expressed the wish that Miss Cameron and I, who were going to test the drugs, might extend our periods in series with the samples, beyond the customary two minutes which I had so far generally found sufficient to get appropriate and significant reactions. His reason was that he had sent me some drugs which should produce primary but might also produce secondary effects, and he wanted to give the latter time to appear. He also requested that we should not delay the tests, as he was not certain of the stability of some of his samples in aqueous solutions. Whilst I was anxious to meet his requirements, I did not fully comply with these two suggestions, firstly, because I had found that with some drugs, even two minutes in circuit was more than a subject could bear, and secondly, because some of my own samples in aqueous solution which I had deliberately left exposed to the air for two years and more were still producing reactions apparently as significant and appropriate as when I had first uncorked them ; a point to which I shall have occasion to return. We therefore planned to take five minutes as our standard period, on the understanding that the circuit would be broken as soon as the subject found his reactions unbearable ; that the observer would call " time " when the five minutes had elapsed, and that the subject would then decide whether he wished to extend the particular test, either because he found it interesting, pleasurable, or beneficial, or because it appeared insufficiently developed.

There was no need for me to point out to Miss Cameron the risks she was running by " taking " twenty-eight different drugs " blind " and in unknown doses, since more than one of our earlier experiments had " upset " her for days. It was not the meagre reward she was receiving which inspired her, but she " would not miss the thrill of these tests for anything in the world, whatever the risks," and I wish here to pay tribute to her courage and devotion.

Miss Cameron, Maby, and I, between us, did 71 different blind tests with our chemist's drugs. To these drugs he added four test tubes about which he would only tell us that they contained " bugs." We divided the 82 test tubes into four groups, and each subject tested a whole group at one sitting. It was arranged that the subject should first rest in circuit without test tubes, and that as soon as he was stabilised, which might take from ten

to thirty minutes, the first drug would be introduced at random. This drug would remain in circuit for five minutes only, after which the next drug would be placed in circuit as soon as the subject declared himself ready for it, and so on through each group of drugs. The subject would describe his symptoms, which the observer would write down, together with his own unspoken observations. There would never be more than a few minutes between the test of one drug and that of the next, and different subjects would experience different "hang over" effects as they passed from one unknown to another in different and fortuitous successions of drugs.

I will read the records of 6* of these 71 blind tests, trusting that they may enable you to accept my assurance that at least 80 per cent. of our tests showed significant and appropriate results :—

Blind Tests. *Sep'ember 9th, 1941.*
Test No. 1. Key Letter "G." *Compare with Test No. 13.*
Subject—M. Cameron. *Observer*—L. E. Eeman.

Time in Mins.		
1.30		Rising breath. Progressively larger and fuller expansions.
2.30	I think stimulating. I ow abdominal breathi g. Arm muscles tensing up as if for effort.	Frowning.
3.00	Beginning in legs.	
3.30	Life in toes. Rather light feeling.	Big sighs.
4.00	Light feeling. Might be able to fly. My body and nothing inside it. Arms and legs hollow. Exhilarating.	Many big sighs.
4.30	Something to do with laughing gas. Glass legs in shop window with light in them. That is how my legs feel.	Laughs. Laughs profusely and stretches.
5.00	*Break* circuit.	
5.30	After having my inside taken out of me I feel cold.	

"G" is : 1 Fluid ounce of solution containing 1 x 1 grain Tablet of THYROID.

On this our chemist wrote : "Particularly successful. Thyroid stimulates metabolism, &c." I may add that this test appears to show an overdose effect.

* It is regretted that from consideration of space 3 have been omitted.

Blind Tests. *September 28th, 1941.*
Test No. 32. Key Letter "R." *Compare with Tests Nos. 39, 48.*
Subject—J. C. Maby. *Observer*—L. E. Eeman.

Time in Mins.		
0.30	May be coincidence; slight flatulence.	Breathing steady. Calm expression.
1.30	Gradually increasing gastric discomfort. Have to swallow. Heartburn. Indigestion coming on.	Looks tense and worried.
2.30	Brow contracting. Tension in cervical vertebrae. Neck drawn back, pillow in way.	
3.00	Face, head and neck all feel set in Plaster of Paris. Feel I must not move or will crack.	
4.00	Feel ought not to turn head to right or left.	
4.30	Slight right ear-ache. Little tingling in skin of upper face. All drawn up and tense. Head drawn back; stiff neck.	
5.00	*Break* circuit. Immediately fall at once as if you had taken off strait jacket. I am free again.	Instantaneous sigh of relief.
6.00	My head has flopped to one side.	Clears throat.
7.00	Everything was gummed up. Could have broken by will. If I had been asleep it could have produced horrible nightmare.	
8.00	Normal almost immediate.	

"R" is: 1 Fluid ounce of solution containing 1 per cent. ADRENALIN CHLORIDE.

On this the chemist wrote: "A marked example of the action of Adrenalin."

Blind Tests. *October 17th, 1941.*
Test No. 64. Key Letter "X." *Compare with No. 50.*
Subject—M. Cameron. *Observer*—L. E. Eeman.

Time in Mins.		
1.00	I am going into a whirl; I am falling.	Breathing fairly full; not tense.

1.45		Smiling, peaceful, a little amused;
2.80		Internal joke ; joke getting funnier.
3.15		Might laugh any minute ; very peaceful.
4.00	Awful effort to speak : first feeling after couple of cocktails ! Pleasant without unpleasant part of being sick !	
5.00		Speech very thick. Laughs and gabbles. Giggles.
	It is funny. I got a bit of a hangover !	
6.00	*Break* circuit.	
6.30	I have a bit of headache. It was funny. I wanted to speak but couldn't. It swept over me within a very little time. I was sinking at once. Headache now.	
8.30	I must stretch. It will be good to.	

"X" is : 1 Fluid ounce containing 10 Drops
Tr. CANNABIS INDICA. MARIJUANA (Indian Hemp).

Our chemist wrote : "Good. Notice the peculiar sensations for which addicts take Cannabis, also unpleasant after effects."

Concerning the test tubes which contained "bugs," Miss Cameron and I had expected that we should show febrile reactions to them. We were therefore very much surprised when, on receiving our chemist's key to his series of numbered test tubes, we found that for each so-called "bug" we had, instead of fever, felt chill and shiver reactions. The explanation was that these tubes really contained anti-febrile vaccines !

After hundreds of blind tests of drugs, vaccines, &c., in series in the relaxation circuit, I have no hesitation in asserting :—

(A) that the propagation of the specific frequencies of drugs, vaccines, sera, &c., by human carrier radiations is a scientific fact.

(B) that they are conducted by electrical conductors in the relaxation circuit.

(C) that when so propagated these specific frequencies cause specific reactions in the human bodies in circuit.

(D) that they do so without the help of artificial energy, and independently of, and even against, suggestion.

(E) that when so propagated, even in minute doses, their therapeutic and also their toxic and overdose effects generally appear in a few minutes, and even seconds, that is, more rapidly than when ingested. Clearly, dosage should be based on time and not on quantity.

(F) that minute doses, kept in imperfectly sealed test tubes, continue to produce the same type of reactions, month after month, without any appreciable loss of power, qualitative or quantitative. This suggests that the inclusion of non-radio-active substances in a human relaxation circuit, endows at least some of them, at least temporarily, with some of the properties of catalysts.

(G) that substances which heal when ingested or injected, also heal when only their specific frequencies are propagated in the relaxation circuit.

(H) that immunity or resistance to disease in one body in circuit also heal other bodies in that circuit.

(I) that, independently of any specific modulation imposed on it by drugs, vaccines, sera, immunity or resistance to disease, &c., the human carrier wave does, unaided, propagate positive health, or vitality, and that it does this without any apparent loss to any of the subjects in circuit.

(J) that, as with many other therapies, the propagation of the "human carrier wave," whether by itself or with superimposed modulations, in the "relaxation circuit," is frequently accompanied and/or followed by more or less violent disturbances, which are evidence of its healing efficacy.

(K) that the propagation of specific frequencies in the relaxation circuit provides us at last with a possible explanation of the specific action of drugs which may account for all the known facts.

(L) that group relaxation therapy is, in fact, true co-operative healing in true human brotherhood.

I will end with a quotation taken from *The Basis of Vital Activity*, the last book written by Sir James Mackenzie, the great heart specialist :—

"That there is an agent, usually called a 'stimulus' or an 'impulse,' which exerts an important influence on all forms of vital activity, has been recognised for centuries. Much speculation has been devoted to the consideration of its nature, with what success can be inferred from the great variety of names now applied to it. Thus it is spoken of as a 'nerve force,' a 'propagated disturbance,' a 'flow of neural energy,' a 'sensory impulse,' a 'motor impulse,' and so forth. The agent itself, in spite of all this babel of nomenclature, would seem to be one invariable phenomenon. It is this last probability which deserves emphasis just now because, during the past years, experimenters have been substituting for this natural agent electric currents

and other artificial means of stimulation. The reactions obtained by these artificial means have frequently been accepted as equivalent to reactions occurring in the normal healthy body and described as 'physiological.' It has not been recognised that the employment of a form of stimulus which Nature herself does not make use of is bound to give reactions which Nature herself does not show. Thus, while, no doubt, information of value has been obtained by the use of electricity, this use has led also to error and confusion. Failure to recognize that a difference exists between this form of stimulation and the natural form has indeed to a great extent prevented understanding of the manner in which organs function.

"What, then, is the natural impulse, the cell impulse, which Nature employs to stimulate or retard the activities of organs? We cannot answer directly, but we assuredly can say that it is one of those forces in Nature which are not themselves susceptible of analysis. Nobody, to-day, expects to make much progress in knowledge of the force of gravitation by trying to find out its essential constitution; the immense additions to knowledge which have flowed from the study of gravitation have been obtained not by direct examination of the force itself but by observation *of the effects which the force produces*. And the same is true of the study of all natural forces. The effects which the cell impulse produces are vital activities. Hence by the study of vital activities we may reasonably hope to arrive at a sound knowledge of their cause.

"In order to accomplish such a research, however, the investigator must start with certain fundamental principles or axiomatic truths in his mind. The principles which we adopted are set out in the following propositions:

"All vital activities are the outcome of the functioning of living cells.

"A living cell is never at rest, being engaged either in discharging its energy or in renewing it.

"The time occupied by the discharge of energy is constant and probably momentary; the time occupied in the renewal of energy is variable.

"Increased activity of a cell is due to a diminution of the period of renewal, and, conversely, decreased activity is due to a lengthening of the period of renewal.

"Agents that affect the activity of the cell act either by shortening or lengthening the period of renewal."

Ladies and Gentlemen, I believe that the phenomenon which I have considered to-day almost exclusively from the "human carrier wave" aspect is the agent referred to by Sir James Mackenzie, and that in the relaxation circuit it does, among other things, shorten the period of renewal of cell energy.

POLAR DETERMINATION AND THE SEXING OF EGGS

By J. CECIL MABY, B.Sc., A.R.C.S., B.S.D.

As a result of various enquiries about egg sexing and recent experiments on "polarity," I wish to elaborate what was said in my short article in *B.S.D.J.*, V., 37.

Note first that (a) it is imperative that the operator should possess a fair degree of radiesthetic sensitivity, and be free from auto-suggested reactions, which are very common and have no objective significance; and (b) the operator has to find out by initial experimentation on a series of objects of ascertainable polarity or sex-connotation what are his own "positive" and "negative," or "male" and "female" responses. Until these two points have been fully determined by repeated trials on both known and unknown objects, it will obviously be unsafe for him to undertake the sexing of eggs. I may say that many people appear to have the potential faculty which probably only needs practice and development.

Second, the operator may find that there is an optimum length of pendulum suspension, a best size and weight of pendulum—for instance, a ball of about one inch diameter of some non-metallic, dry material of a "neutral" kind, such as rubber, bakelite, glass or ivory—and a best hand and pair of fingers for holding the pendulum. All these things must be carefully determined, so that a standard technique may be achieved before the faculty is used for serious purposes. As I have already said, much practice and the elimination of auto-suggested reactions are essential. If the operator gets a certain percentage of failures with specimens used experimentally, he need not despair: the faculty may develop in good time.

Preliminary Experiment

As a test of personal ability, take an ordinary electric battery of several volts potential difference between the two poles (terminals), duly marked + and — or in red and blue. Attach two flex wires, exactly similar in length and appearance, and put the battery in a box with the wires protruding equally outside and not touching, the marking of the terminals being out of sight. Now carry out several trials in succession, on a number of different occasions, in the following manner, noting down the results in four columns headed "Apparent polarity," "Reaction strength," "Actual polarity" and "Remarks." As one tends to tire and become less successful after a few trials, short bursts of work with rests between are always best.

(a) Find the direction of the Earth's magnetic meridian by a compass placed in the middle of a clear room or lawn away from ordinary dowsing reactions due to streams, cables, pipes,

&c. For the site should be "neutral" in the sense that no reactions are normally experienced when standing at the selected spot.

(b) Take the end of the wires between thumb and middle finger of the free hand (it does not matter in which hand the pendulum is held provided the same hand is always used), with the pendulum held horizontally at half arm's length in the other hand, also between the thumb and middle finger, preferably.

(c) Standing on the selected neutral spot, with the whole body relaxed and a blank state of mind, watch the pendulum with mild interest and expectancy, not anticipating any special sort of movement and keeping quite still. Weak oscillations may sometimes result from personal instability or reaction to the Earth's general magnetic field, but if a standard orientation is adopted these can easily be allowed for.

(d) Wait patiently a few seconds for a reaction to start; having started, it will gradually build up to a maximum swing or gyration, but make sure that the latter is not due to draughts or a trembly and emotional hand. So stand firmly with legs a bit apart, and the elbow of the arm holding the pendulum resting lightly on the hips to help steadiness. When a reaction occurs, note its form and intensity under "Apparent Polarity" and "Reaction Strength" (final amplitude of swing). Then see which wire was being held, and note down the "Actual Polarity" and any remarks on the nature of the movement.

(e) Repeat this test many times on different occasions, *being careful that you do not inform yourself of the actual polarity by normal sense perception of any kind.* The eye can be sharp at spotting very insignificant clues, such as a small mark on the battery near one terminal, difference in appearance of two roughly similar wires, &c., once the correct polarity has been associated with one or other terminal or wire. It is, therefore, best in such tests to shuffle the wires by moving them about at random and then to reach out for one of them without looking, so that one can have no idea which is which. Otherwise, the results will be scientifically worthless.

* * * * *

Actual Analytical Tests

In testing actual substances such as eggs, direct contact with the specimen is not essential, as the phenomenon in question is a radiological one; proximity will, in itself, suffice to create some reaction. But the closer the contact the better. And stronger effects may result if specimen and pendulum are held in the same hand.

In tests with an electric battery and similar experiments it is important that the operator's feet and the base of the battery should be at the same electric potential. If both are earthed on

one occasion, but another time only one of them is earthed, reactions may be reversed in the two cases. The operator should, therefore, adopt standard conditions and adhere strictly to them.

The higher the voltage or electric potential difference between the active (" negative ") pole of a battery, cell or organism and the operator, the stronger the pendulum movements will tend to be ; and relative intensity of vitalistic activity and sex differentiation may be roughly measured by a skilled radiesthetist by holding the pendulum, say, over a linear scale (if straight oscillations are obtained) or else a set of concentric circles on a sheet of paper, equally spaced and numbered. The final (maximum) amplitude of the swing can then be read off for any selected object. But note that *standard conditions must be applied, as to type and length of pendulum and above listed factors, if such readings are to possess any physical significance.* Generally speaking, however, a lifeless object and an infertile egg, say, will give little or no response ; whereas the greater the vitality and the stronger the sex phase, the stronger the corresponding reactions, other things being equal. It also appears that the average intensity of the non-vital dowsing rays is governed by geomagnetic and meteorological (variable) factors.

General Discussion

It would appear that when an egg is fresh, but unfertilised (so possessing no electrical imbalance), no polar reaction can be obtained from it ; whereas fertile eggs give a positive or negative phase reaction in accordance with their sex. Some radiesthetists also say that such polarity is indeterminate until the egg has been incubated for, say, 24 hours, when development has begun. This is extremely important, if true, as it suggests that (a) there is a superimposed general cellular polarisation in accordance with sex, and (b) that such preponderance and electrical imbalance is lacking in the sexless organism (*e.g.*, infertile egg, though living and sound), in which one may assume a state of electrical and radiative equilibrium—as delicate galvanometric tests on yolk and white of a fresh non-fertile egg suggest ; the yolk being " positive " and the white " negative," electrically, thus neutralising each other.

Failing a specially set up laboratory, the object of enquiry if not actually held in the operator's hand clear of other contacts should be placed on a dry glass or bakelite plate (preferably coated with shellac varnish to improve insulation) upon a dry plain wooden table, free of metals, &c. And the operator should stand on a similar insulating sheet on a dry floor. Just before each test the specimen should be brought to normal local earth potential by touching it with one finger and placing the other hand at the same time on the floor or ground. *This will remove*

any superficial charges and equilibrate the object and the detector as to general electric charge or potential level. Subsequent reactions may then be attributed to some inherent and " vital " distinction of the kind it is desired to detect. In any case, always keep clear of electric mains and even the outer protective casings of same, which are at an abnormal and " active " potential. Water pipes may be used for temporary " earthing," as in ordinary radio practice.

If these conditions are adhered to, good results will be obtained by a genuine sensitive with remarkable consistence and surety. Otherwise, reactions may be imaginary, confused and worthless, as careful investigators such as Abrams and Boyd have emphasised when discussing so-called " contaminations."

It appears that whereas some operators get clockwise and anti-clockwise *gyrations* of a free pendulum corresponding to the active and passive (male and female or " minus " and " plus ") factors in question, others get (as I do, personally) linear *oscillations*. But this is of no consequence so long as the reactions are consistent and understandable. In any case, whatever system and conditions are eventually found to be most useful individually, adhere to them thereafter.

In computing one's personal successes, be very self-critical, take written notes as suggested above, and tabulate the results. If, for example, the reactions can infer only one or other of two alternative answers, such as " male " or " female," then the chance of fitting the right answer by random luck or guesswork is obviously half of the total number of trials. *So that if one gets only about half right, it means nothing at all*: Hence the need for many critical trials before deciding that one is a genuine dowser—a precaution that very few amateurs take, I am sorry to say. If, however, under such conditions, one scores, say, 60 to 80 per cent., or more, right in 100 trials, then one may be sure of fair success. And it is not exaggerating to say that the really good operator can, and often does, score up to 100 per cent. correct when following such rules as I have tried to lay down in this article.

To conclude: Although these directions may, at first sight, appear tiresome or " unnecessary," careful research and statistical analysis of results since the war have convinced the writer and others of their importance. So the moral is, always aim to be " scientific," whether in great or small undertakings.

DOWSING FIELDS OVER UNDERGROUND SHEETS OF WATER

BY D. O. KING

When considering the problem of depth (*B.S.D.J.*, V., 37), I stated that, in the circumstances, "the fore and aft movement" of a motorscope would not mark reversal of direction of the flow field when crossing a "stream band," since this movement occurs when walking away from a centre point in all directions. At the time I had not suspected the possibility, as is suggested by this article, that a single sheet might throw off a great number of "stream bands" within its limits and therefore in the case of several of these sheets being superimposed and having different directions of flow, a complicated criss-cross system of reaction lines would be detectable at the ground surface. This apart from any edge action around the sheet, supposing that such a thing as a definite edge can indeed exist to saturated beds, especially if they are of a sandy type.

Starting off from a selected spot in any direction, say at a pace of about two miles per hour, with the motorscope held as in figure 4 of Trinder's "Dowsing," it will wobble sideways at once or after a few paces. It is understood that the mind is kept blacked out as regards depth and volume but held to the idea of finding water. Similarly, if the motorscope is held as in figure 5, *i.e.*, not horizontal as in the text but with the point slightly raised, it will at first rise and then dip alternately at about the same places as before. On examining a lin. of pegs driven at the approximate spots where the motorscope moved (extreme accuracy is not necessary), say over a distance of 100 metres, some of the pegs are noticed to lie closer together in groups and on making a return trip with the same hold and at the same speed, the motorscope will mark additional spots about half-way between each original peg. Having driven these new pegs, another more careful run may be made at a slower pace and yet more intermediate points will be found. It frequently occurs, however, when making this more careful run that, especially if some time has elapsed between runs, the motorscope may rise and fall between three pegs instead of rising or falling between the same pegs, *i.e.*, if pegs 13, 14 and 15 have been driven already it will be necessary to replace 14 by two pegs, 14 and 14a. After making further small adjustments, a set of pegs will have been driven, at each of which the motorscope will react as consistently as can be expected (refer to the following paragraph), grip and speed being constant between runs.

I have found it convenient to stop at this stage, because if runs are continued at a slower pace or down to a mere shuffle forward, some hundreds of intermediate points will be found to

exist. I have hubbed up to a possible maximum of 158 within a distance of seven metres. It is not necessary to go to this extreme in practice, since what is required is to peg a just sufficient number of points as will display and not obscure the pattern of "closer groupings" which have already begun to show up along the line of pegs. One or more sets of these may occur, say within 100 metres distance, but the point is that all are consistently evident from the start, no matter how many pegs have been driven. Admittedly, the process looks like deliberate cooking, and is obviously open to a charge of autosuggestion, since, amongst some hundreds of reaction points, it is possible to decide on a system of groupings which may not represent the true facts. However, while bearing in mind other pit-falls such as variation of field strength, lateral shift, &c., I have noticed that they show up the general layout to a degree which, if kept in view, is an efficient guard against autosuggestion provided that the first two runs have been carried out with an open mind. The whole process is really one of catching the manifestation field under steady conditions and while keeping in mind known physical facts, to set out the work in such a manner as will permit of selection of that particular group which, on closr examination, shows the greatest number of reaction points.

Now, if two parallel lines are set out on each side of the original it will be noticed that the pegs of all three lines may or may not fall opposite to each other. If not, they will do so if the central line is rotated and the whole process repeated. Actual example, original direction of march was due North and is finally pegged N.15°E and which I submit marks the true general direction to be taken that will result in a transverse crossing of what appears to be a flow in an underground sheet, depth and volume so far unknown. The direction is classified as general, since, in actual practice, it will be found impossible to bring the corresponding pegs of all three lines exactly opposite to each other except over comparatively small lengths of a few metres. This puzzled me at first, until it struck me that in the case of a saturated bed the general flow would probably be built up of a series of "eddies" between sand particles, each having a slightly different direction of movement. Thus the appearance of the final layout is a zig-zag line, but tending in one general direction.

What is the interpretation of these reaction lines and their groupings? They may be due to R band overlap or perhaps to reactions at the edges of an infinite number of water tables if a saturated bed is to be considered as built up of an infinite number of horizontal planes. On the other hand, the groupings may represent what Major Pogson terms "Stream courses (pressure lines) in the valleys." Refer to "Dowsing over still water" (*B.S.D.J.*, IV., 30). In any case, I have never had

the temerity to recommend a perforation outside any group, selecting always the centre of that group which shows the closest spacing. In other words, my bores have always been drilled in what I take to be the thickest part of the water table, *i.e.*, in the "valleys."

Similarly other directions which will fulfil these conditions can be found by rotating the body round 360° when over the same centre point, but in all cases the pegging will show entirely different groupings. Locally, there are three general directions evident, roughly, N-S, NE-SW, NW-SE, from which it appears reasonable to suppose that three superimposed generalised water tables exist below ground level, each having a different direction of flow. This appears to be approximately substantiated by data obtained from local deep-well borers, but whether I am right in detailing the three tables as follows:

Direction of march N-S; flow W-E; depth 90-180m.
" NE-SW; flow NW-SE; depth 200-250m.
" NW-SE; flow NE-SW; depth about 300m.

remains to be proved. I suspect, however, that many slight variations of direction from the general ones just mentioned may exist and correspond to intermediate depths. There is always the possibility of confusing the radiation and flow fields of superimposed water tables since from the directions given above they lie at right angles to each other. All I wish to do at present is to draw attention to the probability that the dowsing field over an underground sheet does not necessarily consist of a single vertically projected reaction following a hypothetical edge but rather of innumerable bands scattered over the whole surface and usually closely spaced in groups. Moreover, such bands over the so-called "still water" are detectable by at least a motorscope and without the aid of a sample.

Perhaps it may be possible to ascertain the depth, volume and artesian pressure by "scanning" the field, but I have not been able to solve this problem, in spite of intensive study during the last two years. I hope, however, to describe later a method (partly psychological) of estimating the depth and yield of a bore to any one metre's depth down to about 300 metres below ground level. As to whether it can be predicted that the water is fully artesian or not, I can only count on a 50 per cent. success, an even chance which is, of course, useless for dowsing purposes.

In conclusion, I would like to add that, although it might appear to do so, the foregoing does not invalidate my system of depthing as described in *B.S.D.J.*, V., 37, since this consists of a march in any direction from a centre point after "absorption," the motorscope being held vertically at full length of the arms *ab initio*. Incidentally, although I have practically abandoned this method except for depths up to about 50 metres and have substituted it by a new method still to be described,

I do not find it necessary now to walk away from the centre point to a distance corresponding to the depth which it is desired to study. Clearly this should not be necessary, since any reactions received cannot be attributed to a physical cause but rather to a psychological "tuning in" to a depth. Therefore, all that is required is to move forward over a distance of one metre in order to mark on the ground a diagram which shows the saturation conditions at any one pre-selected depth. In a sense the diagram might be considered as a representation of a physical fact and the drawing thereof of a psychological action; in other words, mind and instrument have been combined.

LETTERS TO THE EDITOR

Lower Close,
Quenington,
Gloster.

The Use of Samples in Dowsing

Dear Sir,

I have read, with interest, Mr. Austin Browne's article on "Divining of Ore Deposits" in Vol. V., No. 41, of the *Journal* of the B.S.D., and am endeavouring to answer some of his questions as to the use of samples.

I must begin by explaining that the methods given are those I use myself, and they are, I believe, those more or less in general use, but there may be other methods, used by dowsers, which are equally good. In the first place, it is necessary to understand the reason for the use of samples.

The use of samples enables the dowser, to use a wireless simile, to "tune in" to the metal, or other substance, which he is seeking. A good example of this occurs in the succeeding article, by Mrs. Barraclough, in the same issue of the *Journal*, when she used the leaf of an iris as a sample to show from which plant it had been picked.

In *The Physics of the Divining Rod* Messrs. Maby and Franklin left the use of samples "for future investigation." Having recently been working in close collaboration with Mr. Maby, I think I may say that they have now satisfied themselves not only of the efficacy but also of the *necessity* of their use.

The sample may be held in the hand with the rod or pendulum, fastened over the Solar Plexus, or held in the mouth against the tip of the tongue. With some samples this is, of course, not possible.

In the case of water I always use a small bottle of distilled water, that being the basis of all water.

Similarly, in the case of metals, it is better for the sample to be as pure a specimen as possible of the metal sought, and this will tune in to the basic metal of the ore whether it be Oxide, Sulphide or Carbonate.

If, on the other hand, it is desired to trace the lode of a complex ore, of which one position is known, or to find a lode of similar composition to a known lode, then a sample of ore from the known lode will be of great assistance in locating a similar ore.

As an example I again come back to water. The Abbé Mermet, when tracing medicinal springs, used to use a sample of water from some known medicinal spring, and this would tune him in to that particular constituted form of water.

The above will, I think, answer Mr. Browne's question as to the increase of the dowser's selectivity when carrying a sample.

Clean or rusty nails would, if used as a sample, enable him to detect iron ore deposits in oxide form, and if it is desired to detect iorn pyrites it would be necessary to carry a sample of it.

If, however, the search is only for the purpose of ascertaining if iron, in any form, exists in a particular spot then a sample of pure iron would detect either oxide or pyrites.

Turning to Mr. Browne's remarks on tin and copper lodes in Cornwall, the dowser should, if carrying a tin sample, experience no reaction from a copper lode or *vice versa*. True, mistakes have been, and will be, made, more especially by dowsers with little geological knowledge, but are the geologists infallible?

I may say that I know of at least one big company, in Cornwall, who employ their own dowser, and are generally content to abide by his dictum. In this connection I may point out that here we have the ideal combination, viz.: the dowser and geologist working together.

Some years ago I, using a tin-sample, located four tin lodes in Cornwall running parallel with each other and about 30 to 40 yards apart. Of these, the first three were quite small, but the fourth seemed to be of payable size.

A heading was driven through from an old adit of another worked out lode, and the three small lodes were found to have been correctly located from the surface and the fourth was found to have been worked and "stoped." In the opinion of a mining expert who was present there was more tin at a lower level, but the driving of another heading would have been too costly, so the project was abandoned. Enquiries were made in the locality, but no information could be obtained as to the

"stoped" working, and, as mining memories in Cornwall are long, it must have been worked a great many years ago.

I am not at all surprised that Mr. Browne was not impressed by the experiments he witnessed by television. I took part in these, and the day before the actual television I did a preliminary survey over the buried metals. I was not told if I was right or wrong at the time, but was told, after the television, that all my answers at the preliminary survey had been right. The ground was then quite clear.

On the next day, when coming to do the television survey, I found the ground so covered with high-powered electric cables, twisting in all directions, that I thought I must be suffering from delirium tremens! Any dowser will appreciate the impossibility of accurate work under such conditions, and I was not at all surprised that all my answers were wrong; in fact, I should have been surprised had they been right.

Finally, with regard to Mr. Browne's question as to how the sample of water is to be carried, I can reassure him that it is NOT necessary for him to drink large draughts of water (in fact, this method of carrying the sample might have disastrous results in the case of dowsing for a buried *cache* of wine!); a small bottle of water (corked) held in the hand is quite sufficient.

I used to think that wellingtons or rubber-soled shoes interfered with my reactions but, as I got more sensitive, I find they now make no difference.

Yours,
W. H. TRINDER.

Bower House,
Soberton Lane,
Droxford,
Southampton.
August 31st, 1943.

Dear Colonel Bell,

Mrs. Barraclough's experiment (A Colour Experiment) is of much interest to me, as my own pendulum experiences increase. I must, however, disagree with Miss McKeogh's statement, "The pendulum and rod are but indicators of knowledge already possessed by the Subconscious . . ." Here is an experience of my own, one of many, which refutes the statement. I am not a physician, and what I know of medical work has been obtained mostly by reading. There is no cure for hæmophilia, so the doctors say, and I believe there are no reports of partial cure. I took on a case, 17 months ago without even seeing the patient, and he was cured in three months by a simple herbal

remedy taken twice daily. How could I possibly have had this information in my subconscious? My own belief is confirmed by the pendulum, that the Universal Mind is available for all who are able to tune in to it. Except for giving the approximate time that a treatment will take—the above three months was forecast accurately—I am unable to get forecasts. I do not consider that this is because forecasts are not obtainable but because I am unable to tune in.

(I do not give the remedy as the accuracy of the dose is of vital importance, there is no "stock" strength, and the next patient might not require exactly the same dose as the first).

Yours sincerely,

O. H. BROWNE.

Hazely,
Tring,
Herts.

October 15th, 1943.

Dear Colonel Bell,

Earth-healing

It would be of general interest if some of our members would give their experiences of this mode of natural therapy, and their theories of its explanation. Two cases have lately been brought to my notice. (1) Lady C., after complete exhaustion from climbing in Tirol, lay for an hour supine on the short grass of a rocky mountain side, and rose completely restored. (2) Mr. H., in his garden at Godalming, on gravelly soil, was accustomed to have "two hours on the flat" whenever he was upset, worried, or ill, lying on his back on the bare soil, and invariably rose completely restored to energy and activity, and able to cope with anything.

Lady C. quotes Reinach's *Orpheus*—a history of religions—that the Greeks knew of this earth-healing, and their priests exploited it by giving facilities for invalids to sleep on the earth floor of the temple or in dormitories attached, cures being effected, and attributed, of course, to the God. Also a Russian, Aksakoff, wrote of the power.

Facts and theories would be interesting, of this beneficent action of some soils, contrasted with the maleficent effect of underground streams as so convincingly demonstrated by Frau von Knoblauch in her account in the *Journal* of December, 1938, of her survey of cancer-houses for the Cape Town Health Service.

Yours truly,

ARTHUR MacDONALD.

NOTES AND NEWS

A member has sent us the following interesting communication:

The following passage may be of interest. John Galt was a Scottish author, remembered for his *Annals of the Parish*, and this is from his *Autobiography*, published in London by Cochrane and M'Crone in 1833. Galt founded the Canada Company, and went there as its representative. In this passage he refers to a well he wanted to dig on a farm he was starting, probably in 1829.

"While residing at Burlington Bay, I employed an emigrant to dig a well on my farm, preparatory to other prospective operations. It was seemingly a very absurd proceeding to look for water at the top of a limestone rock, for where I had fixed on building was a mass of that kind, but the project was not quite irrational, nor undertaken without consideration.

"My custom, in the summer evenings, was to prowl alone in the forest. In going along the bottom of the precipices alluded to, I noticed several springs of great affluence, welling from fissures in the rock, and concluded that there were veins and issues of water in the strata, coming from some fountain head. In consequence, I sent for an old German, as famous in those parts for the discovery of ores and springs as Dousterswivel himself, and employed him to perambulate with the divining rod the table land, on which in time I intended to build, in order to ascertain where the well should be excavated. I had no great faith in the operation, but it was one of those things which a person, when he has it in his power, should test.

"I became a little interested, very little at first, in the man's proceedings, but ultimately made him prepare a rod, and took a part in the mystery myself. My own experiment, however, owing no doubt to its being a first attempt, was unsuccessful, but after some time he pitched on a spot for the well, and this was on the top of the artificial mound before described. I determined, however, in spite of reason, to give the German fair play, and accordingly, in disregard of remonstrance, ordered the well to be excavated there.

"After digging through the earthen mass, which was six or seven feet deep, the digger came to the rock, which, with much blasting and perseverance, was penetrated to the depth of between thirty and forty feet, without any symptom of water. At last he reached a porus stratum, in which were the remains of tropical fishes petrified, and the limestone apparently perforated thickly with worms. I have some specimens of this curious subterranean wonder in my possession, but all was yet as dry as hay.

"The well-digger then had recourse to his iron rod, and soon reached with it a subterranean stream of water, which, by rising

through the aperture, half filled the well. I offer no comment on this fact, but the well is there, and obtains its water from the perforated rock."

What would the rod have been with which he was employed "to perambulate"?

It will clearly take far more than a century before Galt's outlook grows common—to test and encourage, although "I had no great faith in the operation."

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A correspondent in the U.S.A. writes as follows:—

Years ago, I am informed, there lived in "Pizen Gulch," in the Blue Ridge Mountain region of southern U.S.A., a man by the name of Noah Buckley, who was locally quite famous for dowsing for water. It is said that the wells he located never failed, and in an account of his work (published 1942) some details of his method are given, which may be of interest considering that the natives of that region have been comparatively isolated and still speak the English of the Elizabethan period which was used when their ancestors settled in this country.

Noah Buckley used a rod of a peach twig, and apparently cut a new one for each experiment, marking the site for the well by simply sticking the baguette in the earth where it dipped. Sometimes sceptics would pull up the twig and replace it with a pebble, but Noah would simply cut another twig and again locate the same spot. He also used a sort of mantra while dowsing that ran as follows:—

"Water, water, if you be there,
Bend this twig and show me where."

He would repeat this over and over as he dowsed *with head bent forward.*

When the youngest of Jake and Norie Mosley's ten children wandered into the woods and got lost, her parents and the neighbours sought till after midnight, and then he came (being sent for), and using as *témoins* one of the child's unwashed garments and a lock of her hair, went into the woods alone and brought her back before daybreak. He took the little garment she had worn, sniffed it, turned it inside out and sniffed it again (getting his "impression," I suppose), and put the garment and lock of hair in one hand with one limb of the rod, holding the other limb in the other hand, clasping both hands tight, dowsing in the darkness.

This account was published in a copyrighted book, *Blue Ridge Country*, by Jean Thomas (Copyright 1942), under heading "Superstition"—much as S. Baring-Gould published the account of Jacques Aymar (1692) under the title *Curious Myths of the Middle Ages.* Apparently if the truth must be given call it superstition or myth, lest it prove useful! Buckley's mantra is interesting since it shows (with his forward bent head) that

he really considered the baguette as an *instrument*, and himself as a sort of *avenue*, via which *under certain conditions* specific actions or phenomena resulted.

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The following striking example of the efficacy of dowsing has recently come to the Editor's notice :

About ten years ago the Managers of an Engraving Works at Watford, Herts, which used a large quantity of water daily, was told that it could no longer be supplied from the local Waterworks. Preparatory to arranging an independent supply, they employed a well-known Norfolk water diviner to locate an underground source. The dowser arrived, and marked out the line of an underground river inside the Company's boundary, indicating three spots where a certain quantity of water would be obtained at a depth of between 150 and 200 feet. Not content with a single opinion, the Managers engaged another dowser, this time from another district, who, to the astonishment of the Works Director, marked out precisely the same line, indicating the depth at which water would be found rather more precisely than the first dowser. A well was sunk and the anticipated supply obtained at a depth within a few feet of that predicted ; for ten years a generous and constant supply of water has been obtained, from 20,000 to 30,000 gallons per hour being pumped from what is now a valuable source.

The formation at Watford is chalk. The line demarcated was obviously that of a fissure. Could a geologist have given better advice, or indeed advice anything like as accurate ?

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The South African paper *Outspan* of July 30th contained an article by our member Mr. J. J. Morton, who has been divining for water and minerals for over 50 years.

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According to the *Nuneaton Observer* of August 13th, a water diviner has been at work at four farms in the Mancetter Hills, which have been taken over by the W.R.G.C. for the production of crops.

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In *Everybody's* of August 23rd there was a full-page, but not altogether accurate, article entitled " Water Divining goes to War."

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In *The Times* of September 10th there was a paragraph from the Special Correspondent at Stockholm stating that the German General Inspector of Water and Energy had issued an order finally forbidding the practice of finding water by divining rods, " as the trustworthiness of the divining rod method had not been scientifically established and therefore cannot be tolerated in Germany, which is the home of exact science " !